publication, Dr. Johnston-Lavis has added one more to the long list of valuable services which he has rendered to geological science.

Les Sciences Naturelles et l'Éducation. Par T. H. Huxley. Édition Française. (Paris: Baillière et Fils, 1891.)

THIS is a translation of various essays with which all English students of Prof. Huxley's writings have long been familiar. Most of them deal with various aspects of the question as to the true place of science in a proper system of education; and no one who has read them in their original form is likely to have forgotten the philosophical power with which the subject is discussed, or the admirable lucidity, strength, and grace of the writer's style. With his educational papers Prof. Huxley has associated his well-known essays on Descartes and Auguste Comte, which cannot fail to be of interest to French readers. He contributes to the volume a short preface, in which he refers with satisfaction to the astonishing advance that has been made in the recognition of science as an instrument of education. He warns men of the younger generation, however, that the battle has only been half won, and that much serious work will have to be done to secure the triumph of the principles for which he has contended. Of the translation it may be enough to say that Prof. Huxley cordially commends it as a faithful rendering of his thought.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

W. E. Weber.

IN the article on Wilhelm Weber (NATURE, July 9, p. 229) no mention is made of the fact that Weber and Gauss in 1833 invented and constructed a telegraph connecting the Physical Laboratory of the University in Göttingen with the Observatory. In Germany they are for this reason said to be the inventors of telegraphy. This is, to say the least, a somewhat sweeping statement, as the possibility of communicating by electricity was known long before that time. However, there is no doubt that Weber and Gauss played some part in introducing telegraphy into practice. For my part I consider the purely scientific work of either of the two men more glorious. For the enormous practical consequences of telegraphy have nothing to do with the scientific merit of the invention. Nevertheless I think that an article on Wilhelm Weber would not be complete without entering into this subject. C. RUNGE.

Hanover, Technische Hochschule, July 13.

[Conducting wires were erected between the Göttingen Observatory and the Physical Cabinet of the University, distant about three-quarters of an English mile, in order to obtain accurate comparisons of the clocks. But, in addition to systematic daily transmission of time, the wires were from the first frequently used for telegraphic purposes, though, with the first arrangements, only two letters could be sent in a minute.—G. C. F.]

Earthquake Shocks in Italy and Australia.

BY a telegram from London, which appeared in the newspapers here on the 12th and 13th inst., information is given of a severe earthquake in Italy (about Vesuvius) on June 7 (Sunday). On that day, several distinct and well-marked shocks were felt over parts of the south of Australia, and as there may be some connection between these seismic disturbances in both hemispheres, I give below the times and other information of the disturbances experienced here.

None of the disturbances reported in Australia seem to have been more than a "shake" or *sharp tremor* sufficient to shake windows and rattle crockery, &c., but they were enough, in some instances, to produce feelings of nausea.

June 7, at 2.5 p.m., the first disturbance occurred, and was felt all around Melbourne and over a surrounding area of 360 square miles.

June 7, at 2.45, another shake (not so great as the first) was also felt; in this case it was felt most severely to the east of Melbourne.

June 7, at 7.20, smart shock felt at Kapunda, South Australia.

June 7, at 6.45, slight shock felt at Stockport, South Australia. The direction of motion is variously given as from north-west to south-east, south-east to north-west, and south-west to north-east, north to south, south to north, &c. The conclusion arrived at is that the wave was from south to north nearly. The approximate geographical positions of the several localities where these disturbances occurred are as follows:—

	Lat.	Long.
	° ′	° ′
Stockport	34 21 S.	138 57 E.
Kapunda	34 21 S.	138 46 E.
Melbourne ...	37 50 S.	144 58 E.

Melbourne, June 15. R. L. J. ELLERY.

P.S.—It is quite probable the shocks felt at Kapunda and Stockport were one and the same, as time is not very strictly kept in districts distant from large towns in Australia.

Force and Determinism.

I SEE nothing to criticize in Mr. Dixon's middle paragraph, wherein he accurately summarizes some of the definitions of mechanics, except that I should prefer to express the meaning of his last sentence by saying that, if in any department something simulated the functions of, say, energy, without obeying its precise mechanical laws, then the distinction between energy and that something should be clearly recognized, and another name be given to it.

I find it rather common for "life" to be thought of and classed under the head energy, either by the use of a phrase such as "vital energy," or in a more direct way; the reason being apparently that organisms while living simulate some of the functions of energy, and cease to do so when dead. It was against this confusion that I wrote on p. 491 (vol. xliii.).

Life has not yet been included in the domain of physics, neither has it, so far as I am aware, been much studied under the head biology.

And yet the disturbing action of live animals will have to be formulated and attended to some day, even in physics; for, though they generate no energy nor affect its amount in the slightest degree, they certainly control it and direct it in channels it would not otherwise have taken. The question is, How do they manage this? And one answer that may be given is, By exerting directive or guiding forces on matter.

Of course they are not limited to this, but in so far as they do work their action is fairly understood: the energy displayed by a gang of navvies is known to be derived from the little tin cans they bring with them: the energy is not theirs but their victuals', they simply direct it. But how comes it that they can direct the energy of victuals and atmosphere into the erection of the precise bridge or other structure which has been planned? What determines the direction of the transfer of energy?

The same question may doubtless be asked in connection with inanimate activity: I would not be understood as assuming for certain any clear or essential difference between the two cases; but in neither case do I know the answer.

The action of force in doing work, *i.e.* transferring and transforming quantities of energy, has been thoroughly attended to.

The action of force in directing and guiding the transfer of matter and energy does not seem to me to have been seriously contemplated.

In his most recent book ("The Philosophical Basis of Evolution") Dr. Croll attacks the problem, and says that guidance is effected by "determinism" not by force. But that cannot be admitted; for without force the motion of matter cannot be changed in direction any more than in speed. *Force* is certainly necessary to direct the motion of matter, it is *energy* only which is unnecessary; for any transfer of energy that may occur is an accidental, not an essential, concomitant.

I determine to move an object: it may be only my finger, or it may be a wheelbarrow. In so far as I do any work in the action I do so at the expense of my food, and there is nothing but a chemical difficulty about that. The mystery begins when one

©1891 Nature Publishing Group

asks how I manage to direct that energy along a definite path so as to produce a willed result. The only answer I know is, "By a nervous impulse liberated from brain centres." But what is it that is thus liberated? and what pulls the trigger to liberate it?

By mechanical analogy one would say that energy can only be guided by force, and that force must therefore be exerted in the brain cells; but, if so, the relation between force, which is a mechanical thing, and will or life, or whatever it is, which is a psychological thing, demands investigation.

I trust that Mr. Lloyd Morgan will help me to get my ideas on these subjects straighter, and will point out if I have made any assertions which are obviously erroneous or grotesque. The borderland of psychology and physics is the last place in which I would like to dogmatize; and in a letter like this I see no harm in airing confessedly immature and groping notions, in the hope that ventilation may clear the air. So far as physics only is concerned, I have stated how I regard the phrase "expenditure of energy" in the *Philosophical Magazine* for June 1885.

With regard to the crux raised in Mr. Dixon's last paragraph, that nothing but matter can exert force, because the acting matter must receive an equal opposite momentum, it may perhaps be just worth noticing that an infinite mass can absorb any amount of momentum without receiving a trace of energy or being itself in any way affected.

OLIVER J. LODGE.

Liquid Prisms.

I OBSERVE in NATURE of July 2 (p. 207), that it is stated Herr Wolter has recently recommended α-monobromnaphthalene as a substance peculiarly fitted for study of the ultra-violet part of the spectrum, by reason of its high dispersive power and transparency for the ultra-violet rays.

Perhaps I may be permitted to state that Mr. Madan published an account of its dispersion and refractive power in the *Phil. Mag.*, and recommended its use in liquid prisms. Having made use of many other substances, including methyl salicylate, I gave this a trial. For ordinary work it would be excellent if colourless; but unfortunately, no matter how free from colour it may be when freshly prepared, long-continued use causes it to become yellow, and in considerable thicknesses even dark brown. For the ultra-violet rays it is undoubtedly better than carbon disulphide, but nevertheless practically useless, as the line N, which it is said to transmit, has a wave-length of 3580, so that only about one-half of the ultra-violet solar rays are observable with it. In metallic spectra almost all lines of interest lie between 3580 and 2000. A liquid which I considered to possess much superior optical properties is *mercuric methide*; it is perfectly colourless, and of such density that flint glass will float upon it. When the glass is immersed it becomes invisible, consequently the refraction and dispersion of the liquid are probably exceptionally high. As far as I can recollect, being without access to my notes, a thickness of 50 millimetres freely transmitted all rays to about λ 2900—that is to say, the entire solar spectrum. Unfortunately, it has its drawbacks, in being somewhat volatile, and its vapour highly poisonous.

Stonehaven, N.B. W. N. HARTLEY.

The Identification of Templeton's British Earthworms.

BETWEEN the years 1829 and 1836 the first series of Loudon's *Magazine of Natural History* appeared in nine volumes. In the last volume we find some notes on earthworms by Templeton, which have proved somewhat puzzling to students of more recent times. I have been fortunate enough to follow Templeton in some of his researches, and am able to correct and verify certain of his statements.

The *Lumbricus xanthurus*, Temp. (*op. cit.*, ix. 235), is the angler's gilt tail, and as such is synonymous with *Lumbricus puter*, Hoffm., and *Dendrobæna Boeckii*, Eisen. *Lumbricus gordianus*, Temp. (*loc. cit.*), is undoubtedly the mucous worm (*Allolobophora mucosa*, Eisen), or one of its near allies, all of which are to be found of a pale rosy colour coiled up into a knot at certain times of the year.

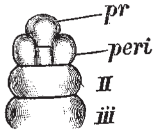
It is to *Lumbricus omilurus* (= *Omilurus rubescens*, Temp., *loc. cit.*), however, that I wish to direct special attention. Grube, in 1851 ("Familien der Anneliden," p. 101), placed it, with Templeton's other worms, in a list of species which were insufficiently characterized for systematic purposes. Vejdovsky, in 1884 ("System und Morph. der Oligochæten," p. 62), places it among the questionable species without note or comment, and, so far as I can gather, no one has been able to throw light upon it since.

Templeton says the worm is never larger than half the size of *L. terrestris*, L., is of a bright reddish-brown, with the tail very flat, and the body *unfurnished with a belt at the position of the sexual organs*. It would be very easy to suppose from this somewhat vague account that the writer had only seen immature specimens; but a little careful study of his words shows that he knew what he was writing, and that his worms were mature. Now a mature species of *Lumbricus* without a clitellum is certainly an anomaly, and needs investigation.

While collecting Annelids recently, I came across half-a-dozen specimens which at first sight exactly resembled *Lumbricus rubellus*, Hoffm. I took them home for verification, and immediately observed the difference. I had obtained with them typical specimens of *rubellus*, which enabled me to make a careful comparison of the two species in a living state.

The following is a description of the worm as I wrote it down before observing Templeton's account.

Colour dark brown, iridescent on the dorsal surface anteriorly, becoming lighter towards the posterior extremity, which is flesh-coloured or light red, pink ventrally. Prostomium dovetailing completely into the peristomium, and possessing a transverse groove in the middle, as shown in the accompanying sketch.



Lumbricus rubescens. Segments 1 to 3 with prostomium entirely cutting the first segment or peristomium.

Segments not annulated (or divided by transverse rings). Length about 3 inches, total number of segments about 120. Setæ in couples as in typical Lumbricus. Male or spermiducal pores on segment 15 with papillæ, which, however, do not extend over the neighbouring segments. Body cylindrical in front, flattened posteriorly. The dorsal pore between 5 and 6. It appeared at first between 7 and 8, but by using polarized light on the cuticle when spread on a glass slip the whole series of pores in one or two specimens became clearly visible from the fifth segment backwards.

On the ventral surface prominent papillæ appeared on segments 28 and 29, such as are often seen on typical *L. agricola*, Hoffm. Now came the crucial question, Is there no clitellum? By studying all the examples carefully, I found that they agreed in one particular. The segments 34 to 39 differed in structure from the rest on the dorsal surface. On the under surface from 33 to 40 were differentiated, and showed a glandular structure, while the band representing the tubercula pubertatis extended distinctly along the ventral surface of 35, 36, 37, 38.

This description of the external characters shows the worm to be a decided Lumbricus, tested by Dr. Benham's definition in "An Attempt to Classify Earthworms"; but it differs from every one of our British species, especially in the backward position and inconspicuous nature of the clitellum. I am unable to refer it definitely to any of the European species, and propose that for the present it should be known as *Lumbricus rubescens* (Temp.), thus retaining the two names from Templeton's synonyms which are most appropriate to what I regard as the species intended by him.

I may add that I have recently found one or two other earthworms in Yorkshire which have not yet been recorded as British, and will form interesting additions to our Annelid fauna.

Idle, near Bradford, July 15. HILDERIC FRIEND.

Copepoda as an Article of Food.

DURING recent years a good deal has been said amongst marine zoologists of the use, as a food supply, that might be made of the enormous numbers of Copepoda that swarm in the surface-waters of the sea, and the Prince of Monaco has pointed out the value this widely-distributed nutritious matter might have to shipwrecked sailors; but I am not aware that anyone has yet actually made the experiment of cooking and eating Copepoda, so the following record may be of some interest.

©1891 Nature Publishing Group